

January 14, 1965

Dr. Seymour S. Kety
National Institutes of Health
Bethesda, Maryland 20014

Dear Seymour:

I must apologize that it has taken me a couple of months to crystallize a more definite reaction about the manuscript you sent me last summer on "Memory", and I hope you don't confuse that procrastination with lack of interest.

I find myself in tenacious agreement with the position that you presented and feel that it is rather important to sustain broad interest in a conservative approach to research on memory, conservative in the sense of adhering to a well-established tradition as against running down every flight of fancy like, for example, the planaria work. I was going to say that since an immense amount of very hard work and inspired thinking is going to be needed to unravel the structure of the logical network, it was very undesirable to have a major distraction from this effort. However, sometimes I think that it can be an advantage for a scientific field not to be over-populated, provided, of course, it can retain sufficient glamor to be able to command the support necessary to continue its effective development. Howbeit, I think that we nevertheless share some sense of provocation at the RNA-sequential theories, and I would be very pleased indeed if we could find some common channel of expression.

S
Kety

What puzzles me now, though, is exactly what I could contribute to your lecture. As a lecture it is a model of its kind, and I would seriously hesitate to tamper with it in any way. This is certainly one approach I could wholeheartedly recommend to you, and it has the advantage that you could see to having it published without any further effort on your part and with no intrusion from me. I suspect that a journal like the American Naturalist or Perspectives in Biology and Medicine, perhaps Science, would be very happy indeed to receive such a communication in just its present form.

On the other hand, speaking strictly stylistically and on a matter of purely personal preference, I don't think I would be capable of writing such a clear exposition that would be so readily intelligible to a wide audience as you have done, and if I were to be attempting a similar task myself I would be false to my own tradition to leave an article in that state of clarity. Instead, I would find myself trying to take out all of the light-hearted but meaningful jokes and trying to densify the presentation into its most economical, rather than necessarily most clear and persuasive form of statement. So here is the dilemma, and what would you care to do? In many ways the first proposal might be the most sensible one, especially from the standpoint of expected yield per unit effort. It would gratify me somewhat more to be able to work with you a little more closely on a more sophisticated version, but I would say right out that if we want the paper to be clearly understood, you had

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better take the reins with regard to its general style and while the present text might benefit from some small amount of wringing out, it would be better not to let my reflexes go to work on it in that way. I do have attached a couple of memoranda that, if you care to go down this path, you might want to interleave in the text.

So now I suppose I will have succeeded in throwing the ball back to you. I think I can promise that if you do want to take this up, I will have a few more things to add, somewhat along the lines of the attached memoranda, and that particularly, I will be moved and able to respond rather quickly and not along the lines of the rather disgraceful delay that preceded the present letter.

and

Dave Hamburg probably told you, /I would just reinforce it, how we and several others independently of us here at Stanford were chagrined by that recent atrocity by Huxley, Mayr and Osmond that appeared in Nature on "The Genetics of Schizophrenia". In fact, we were boiling mad. While, after having just read Julian Huxley's rebuttal to a book review by Mirsky in the last Scientific American, I am still even a little less surprised about him, I find it utterly incomprehensible how Ernst Mayr could have been roped into such nonsense. I gather, however, that some response will be forthcoming, and I do think there are some issues that can have such græevous effects on important policy that we cannot afford to let them go unchallenged.

With best wishes,

As ever,

Joshua Lederberg
Professor of Genetics